theodolite, sextant, &c. In the section on the globes, a short summary is given of ancient and curious globes, succeeding which are descriptions showing how latitudes and longitudes, day and night, &c., are measured, also the principles of great circle sailing. Part iv. deals with map-drawing, in which a brief but plain description is given of the various methods of projection, such as orthographic, gnomonic, stereographic, conical, &c.; reference also is made to the different symbols used in map-drawing. The last two parts treat of map copying and memory-maps, of which the latter will be found of great importance, for, by using the method adopted, and carrying out the suggestions, the learner may remember much that might otherwise be forgotten.

Krystallographisch-chemische Tabellen. Von Dr. A. Fock. (Leipzig: Wilhelm Engelmann, 1890.)

THIS little work of ninety pages supplies a much-needed want in chemical crystallography. The increase of late years in the number of original memoirs, describing the crystallograpical characters of newly-discovered chemical compounds, and pointing out relations between many of the longer known ones, has been so great that the text-books now in use, such as that of Rammelsberg, are very much out of date. Dr. Fock presents the data acquired up to the present in a very condensed and easily referable form; a form, moreover, which at once exhibits such relationships as have been noticed between chemical composition and crystalline form. The tables will be found to include brief descriptions of all the more recent measurements contributed to the *Zeitschrift für Krystallographie*, as far as regards the crystalline system and the elements of the crystals measured. The arrangement adopted is somewhat similar to that in Groth's "Tabellarischer Uebersicht des Mineralien," but including in addition almost all the known chemical salts, and all the measurements of organic compounds yet made. The information furnished concerning each of the compounds mentioned consists of (1) its symmetry, (2) the ratio of its axes, (3) the axial angles of monoclinic and triclinic compounds, and (4) the observer by whom the measurements were made. In many places, where important relations have been noticed, additional information of a character extremely useful for lecture purposes is given. It is quite evident from the whole character of the work that great care and a large expenditure of time have been involved in its preparation, and those who are interested in the subject must feel greatly indebted to Dr. Fock for collecting such a valuable store of information in so handy a form.

A. E. TUTTON.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

Mr. Wallace on Physiological Selection.

BY his second letter Mr. Wallace leaves no possibility of doubt touching (1) the manifest agreement, and (2) the alleged difference, between his recent theory of cross-infertility in relation to the origin of species, and the preceding theory on the same subject, as published by Mr. Catchpool, Mr. Gulick, and myself.

(1) The manifest agreement consists in supposing, as he says, that some amount of infertility characterizes the distinct varieties which are in process of differentiation into species; and that such "incipient infertility" is of so much importance in this "process of differentiation" that its absence may be regarded as one of the *usual* causes of the failure of varieties to become developed into distinct species.

(2) The only point of difference alleged is, that while Mr. Wallace says this incipient infertility can *never* arise alone, or except in association with some other and preceding varietal difference, we (according to him) have represented that it must *always* arise "alone, in an otherwise undifferentiated species," and therefore always constitute the *initial* change in the way of varietal divergence.

Such being the only point of difference alleged, it is obvious in the first place that the allegation, even if valid, has reference to a point of but secondary importance. For if we are all agreed that the "incipient infertility," whenever it does arise, is a factor of such high importance in the origination of species as Mr. Wallace now admits, surely the question whether it can ever arise before (or can only arise after) incipient varietal characters of any other kind becomes a question of comparatively little consequence. But now, in the second place, the allegation is not valid, being, in fact, the very opposite of the truth. Taking first my own presentation of the theory, both in "the original paper and in the summary of it published in NATURE," I not only expressly stated, but carefully argued, that the incipient infertility may arise *either before or after* variations of any other kind; and, in order to emphasize this distinction, I devoted one part of the paper to the first class of cases, while relegating to another part my consideration of the second class. Therefore it is merely by an eclectic method of quotation that Mr. Wallace now represents that I began by setting forth only one side of "physiological selection," or the cases where incipient infertility is the prior change. Why he should persistently ignore all the other part of the same paper, or the cases where I show that incipient infertility need not be the prior change, I do not care to inquire. But at least the omission cannot be due to any want of clearness on my part, inasmuch as in his first criticism of the paper, which he published several years ago, he displayed a complete understanding of what I had said upon this point.[1]

After this much explanation it seems almost needless to say that I stand by every one of the "eight quotations" which Mr. Wallace has given. For "it [still] appears to me much the more rational view that the primary specific distinction is likewise, as a rule, the primordial distinction; and that the cases where it has been superinduced by the secondary distinctions are comparatively few in number;" "it is [still] on what may be called spontaneous variability of the reproductive system itself that I mainly rely for evidence of physiological selection;" I still continue to ask, "Why should we suppose that, unlike all other variations, it [*i.e.* the physiological variation] can never be independent?" And so on through all the eight selected sentences, provided that any regard at all be paid to their context and relation to other parts of the paper. For no one of these sentences in the smallest degree affects the position which from the first I have consistently and persistently held—viz. that it makes no difference to the theory in what proportional number of cases the physiological change has been the prior change. Indeed, the *immediate* context of the first of the above quotations sets forth that it would make no difference to the theory even if we were to suppose that in *no* case can the physiological change have been the prior change.[2] In other words, it is expressly stated that, even if we were to adopt the identical opinion on which alone Mr. Wallace now relies as constituting any difference at all between his theory and my own, still the latter, in its "principle" or "essence," would be in nowise affected. Yet Mr. Wallace now accuses me of "an absolute change of front" on the sole ground that I repeat these statements![3]

So much for my own paper. Mr. Catchpool's enunciation of the theory was much too brief to admit of any fair criticism of the kind which Mr. Wallace now passes upon it. But Mr. Gulick's elaborate essays—which he abstains from mentioning—are quite another matter; and, as stated in my last letter, they considered much more fully than mine had done the subordinate

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[1] *E.g.*, "Mr. Romanes then goes on to argue that, as a rule, these physiological variations are those which occur first, and form the starting-point of new species. He admits that in some ['possibly in many'] cases sterility may be a secondary character, due perhaps to the constitutional change indicated by the external variation; but even in that case physiological selection plays an equally important part, because if it [*i.e.* the incipient infertility] does not arise, either coincidently with the ordinary external variation, or as a consequence of it, then that variation will not be preserved, but will rapidly be extinguished by intercrossing with the parent form" (*Fortnightly Review*, 1886, p. 302). This brief extract is enough to show how widely Mr. Wallace's first representation of my "original paper" differs from his last, as regards the only point now in question.

[2] See NATURE abstract, vol. xxxiv. p. 339.

[3] There are several other distortions of my views in Mr. Wallace's letter, but space prevents me from dealing with them.

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question at present before us. In the result Mr. Gulick completely agreed with me, that it cannot signify *how* or *when* the physiological variation of initial cross-infertility arises; for to whatever *causes* it may be due, and at whatever *time* in the process of varietal divergence it may first occur, it must alike furnish as highly important a condition to the origination of species as Mr. Wallace has eventually himself assigned to it.

I say "eventually," because Mr. Wallace has never before expressed himself to the effect that, in his opinion, cross-infertility is a factor of such prime importance in the origination of species. Why has he never done so? Surely the matter is one of sufficient magnitude to have justified some mention in one or other of the many valuable "contributions" which he has made to the theory of evolution. Or, not to go further than his past criticisms of my own paper on the subject, what pages of controversy he might have saved in this journal and elsewhere by stating, at any time within the last four years, that he had no disagreement with me touching the probable occurrence, and the important consequence, of some degree of infertility characterizing varieties which afterwards, and on this account, develop into species; but merely doubted whether any degree of infertility could ever arise before differentiation of some other kind had begun to take place. Such criticism would have been mild indeed. But hitherto the crown and front of opposition to the theory of physiological selection has been that, in representing cross-infertility as a factor of any great importance in the origination of species, the theory is not only untrue in itself, but tends to "shrivel up natural selection to very small dimensions." Now, however, criticism "changes front." It is no longer denied, but actually upheld, that "selective fertility" is as highly important a "co-operative cause in the origination of species" as I have ever claimed; and the new attack is directed only to a very subordinate point—a point, moreover, which both Mr. Gulick and myself had expressly anticipated, fully discussed, and shown not to belong to "the essence of the theory."[1]

Oxford, December 22, 1890. GEORGE J. ROMANES.

Molecular Dispersion.

IN the notes that appeared in your journal of December 11 (p. 133), you gave a very full account of some papers lately published in the *Bulletin de la Société Chimique de Paris*, on optical dispersion, by Messrs. Barbier and Roux.

This investigation is a remarkable instance of how laborious and intelligent work may be almost wholly thrown away for want of the knowledge of what has been previously done in the same direction. The authors commence their first paper with the astounding statement: "Dispersion has never been studied from the point of view of the relations which connect this physical property of bodies with their composition, their molecular weight, and their chemical constitution." This, however, was attempted by Sir John Herschel more than half a century ago, though with little success: and most of the scientific men who have best elucidated the subject of molecular refraction, such as Mascart, Brühl, and Nasini, have paid some attention also to dispersion. Mr. Dale and I gave numerical values for dispersion equivalents, analogous to Landolt's "refraction equivalents," for CH_2, Cl, &c., as far back as 1866. The dispersion of isomeric bodies was treated by me in 1881; and within the last few years I have communicated papers on the subject of molecular dispersion to learned Societies in England, France, and Switzerland. Some of the substances worked on by Messrs. Barbier and Roux have not, I think, been optically examined before; but the value of their careful observations is much diminished by their having measured two lines of the tin spectrum, instead of the A and H of the solar spectrum, or the α and γ of that of hydrogen; thus their results cannot easily be compared with the hundreds of measurements of dispersion already published by others. They have unfortunately employed Cauchy's formula, and have taken as the "specific dispersive power," $\frac{B}{d}$; that is, Cauchy's B divided by the density of the substance, instead of $\frac{n_a - n_b}{d}$, the difference between the refraction of two lines divided by the density. Brühl commenced to work in the same way, but shows in a paper in *Liebig's Annalen* for August 1886 that the method is unsatisfactory. The generalizations of our present authors are definite, and apparently correct, but they could have been mostly foreseen and explained if they had more clearly grasped the idea of molecular dispersion.

In the early summer I sent Messrs. Barbier and Roux copies of my papers on the subject; on August 1 they acknowledged receipt, and promised to refer in a forthcoming paper to previous work. As the paper reviewed in NATURE appeared in a preliminary abstract in the *Comptes rendus* of July, it is evident that they have yet something to bring before the public.

J. H. GLADSTONE.

17 Pembridge Square, December 27, 1890.

Weighing with a Ternary Series of Weights.

IN a former communication (NATURE, November 13, p. 30) I omitted, in order to avoid prolixity, to show how readily any given number may be expressed in the notation therein proposed. To effect this, it is only necessary to express the given number in the ternary scale, and then, beginning on the right, to substitute for 2, wherever it occurs, − 1, whilst increasing the previous figure by 1. When 3 occurs in the application of this rule, it must of course be replaced by 0, the previous figure being again increased by 1.

Examples:—

41 is in ternary notation			1112,	in new notation	**11111.**
500	,,	,,	,, 200112,	,, ,,	**1101111.**
71	,,	,,	,, 2122,	,, ,,	**10101.**

Or (still more briefly) in dividing by 3, to express the number in the ternary scale, we may substitute for the remainder 2, wherever it occurs, − 1, and increase the quotient by 1.

Examples:—

425			474			500		
142	remainder	− 1	158	remainder	0	167	remainder	− 1
47	,,	1	53	,,	− 1	56	,,	− 1
16	,,	− 1	18	,,	− 1	19	,,	− 1
5	,,	1	6	,,	0	6	,,	1
2	,,	− 1	2	,,	0	2	,,	0
1	,,	− 1	1	,,	− 1	1	,,	− 1
0	,,	1	0	,,	1	0	,,	1
$3+27+729$ $-(1+9+81+243)$			$729-(3+9+243)$			$27+729$ $-(1+3+9+243)$		

Bradford, December 13, 1890. J. WILLIS.

PROF. EVERETT's rule (NATURE, December 4, p. 104) is needlessly complicated. All that is necessary is to express the given weight in the ternary scale with digits that are either 0, + 1, or − 1. His example is thus solved:—

$$
\begin{array}{rl}
3\overline{)500} & \\
3\overline{)167} & -\ 1 \\
3\overline{)56} & -\ 1 \\
3\overline{)19} & -\ 1 \\
3\overline{)6} & +\ 1 \\
3\overline{)2} & +\ 0 \\
1 & -\ 1
\end{array}
\qquad \text{or } 500 = 1\bar{1}01\bar{1}\bar{1}\bar{1} \text{ ternary}
$$

$$= 729 - 243 + 27 - 9 - 3 - 1.$$

We thus see generally that, to express weights in units and powers of n, we must be provided with $\frac{1}{2}n$ or $\frac{1}{2}(n-1)$ balance-

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[1] P.S.—Mr. Wallace alludes to my "standards of scientific reasoning and literary consistency." As regards the former, I am satisfied with a full and independent corroboration by a consistent and a logical mind. As regards the latter, it is enough to quote the concluding words of my reply to Mr Wallace's first criticism of four years ago:—"The *main* feature of the theory is what my paper states it to be—viz. that sterility with parent forms is one of the *conditions*, and not *always* one of the *results*, of specific differentiation. But, if so, is it not evident that all causes which induce sterility are comprised by the theory, whether these causes happen to affect a few individuals sporadically, a number of individuals simultaneously, or even the majority of an entire species?" (*Nineteenth Century*, January 1887). And is it not equally evident, as elsewhere stated, that it does not signify whether the sterility arises before or after the "differentiation" has begun; seeing that, in either case, without the sterility the differentiation (as Mr Wallace now says) will usually fail to proceed to the formation of distinct species? I have no space to discuss Mr. Darwin's views on this subject; but assuredly they are far from those which are expressed either here or in "Darwinism."

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